

the omission of any mention of the therapeutic value of electricity in rheumatism, articular and muscular, which is in our opinion, well established.

In our notice of some of the defects, or what appear to us to be such, of Dr. Wood's book we do not wish to convey the impression that we have not a very high opinion of its value and usefulness. It is, in many respects, the best text-book of its kind that we have as yet met with, and the only one in our language that has struck out decidedly in the right direction. It may be called a treatise on rational physiological therapeutics as distinguished from those that follow the more exclusively empirical methods in the ordinary medical sense and usage of the word.

We know of no better book in the English language on this special subject, to recommend to the student or the practitioner. The rapid exhaustion of the first edition was of itself a sufficient evidence that it met a want of the profession, and we trust that a third edition will be speedily required.

IV. THE VASO-MOTOR NERVOUS APPARATUS.*

LECONS SUR L'APPAREIL, VASO-MOTEUR (PHYSIOLOGIE ET PATHOLOGIE) Faites a la Faculte de Médecine de Paris. Rédigées et Publiées par le Dr. H. C. Carville. Tome Second. Paris, 1875. P. 775. (*Lectures on the Vaso-motor Nervous Apparatus, etc.*)

Continued from the last number of the JOURNAL.

M. Vulpian continues his study of the subject of the temperature of the animal body, especially as affected by active changes in the size of the bloodvessels. The influence of *pain*, on animal temperature is considered. It is lowered as a rule under the influence of pain, even if the animal is under the influence of woorara, according to Mantegazza, Heidenhain and others.

But this conclusion is denied by others, especially by Riegel, in a series of very elaborate investigations published chiefly in *Pflueger's Archiv*. But the earlier views of Heidenhain have been re-affirmed by Ostroumoff, in a recent article in *Pflueger's Archiv* for March 1876.*

*Versuche ueber die Hemmungsnerven der Hautgefasse Von Dr. A. Ostroumoff, aus Moskau. *Pfluegers Arch.* March 1876.

In a brief discussion of the part the vaso-motors play, in the phenomena of fever, M. Vulpian combats certain conclusions arrived at by Schiff of Florence, and concludes that the vaso motor nerves have but little part in the mechanism through which bodily temperature is augmented in fever. "The nervous centres," he says,—“must, as I have endeavored to make clear to you, act more directly on these phenomena, either by the nerves of animal or organic life, which are placed in a more or less immediate relation with the anatomical elements, or in a more general manner with the living organized substance of the different tissues. Or we may admit finally, that the morbidic agents, (pyretogenic agents) may act immediately on the tissue substance itself, so as to modify directly, the nutrition and thermogenic processes, and hence without the intermediation of the nervous system.” P. 265. We have no hesitation in expressing our preference for the view which admits for the higher animals at least, a direct connection between certain of their nerves, and the anatomical elements of the higher tissues. But if it were in the plan of this notice to enter on a discussion of this subject, we have not the space at present.

We believe, that in the fever process, the elevation of temperature is due to increased tissue change, and that this change alike in disease and health, while it does not depend on, is, or may be, profoundly influenced by the nervous system. The cause of the remarkable elevation of the temperature of the body, which occurs at times after death is spoken of as follows: “The cause of this *post mortem*. elevation in central temperature, is not easy to determine. In this connection, it is necessary to remember that the death of the individual occurs before that of the anatomical elements, which compose its tissues. These latter continue to live, although the circulation and respiration have entirely ceased. If they continue to live, it is because the processes of nutrition take place in them as before, and hence the phenomena of intimate combustion continues, upon which the production of bodily heat depends. There is, therefore, no radical impossibility in admitting that heat may be produced in the interior of the body after death, and while the ultimate anatomical elements preserve a share of their proper nutritive energy.” P. 276. Besides, M. Vulpian, as it seems to us, correctly supposes that the rise in temperature after death is due not so much to a more active production of heat, as to its *retention* at the point of production, by reason of the stoppage of the circulation which occurs at death. While it continues, the blood becoming heated at one point, becomes cool, by a transmission through the cooler superficial parts of the body, and in this way the heat produced at one place, is borne to others.

M. Vulpian does not admit the existence of a “moderator centre,” either in the medulla oblongata, or any other part of the nervous system, as claimed by Dr. H. C. Wood, of Philadelphia, and others.

The causes of the lowering of temperature, in surgical shock, and which follows extensive burns, are discussed in an interesting manner, but we do not have space at present in which to analyze the reasoning and results, and besides this they are not quite germane either to the subject of the work, or of this notice. This is one of the most conspicuous features of M. Vulpian's work, viz.: the proneness to discuss subjects outside of its formal limits.

The next subject considered is that of the "influence of the vaso-motors on intimate nutrition." This section is introduced by some admirable remarks on the process of "intimate nutrition," which embrace not only the sum of our positive knowledge, but also in part, what we may reasonably infer, in respect to that process, in its various aspects, as it is accomplished alike in plants and animals. What influence could we reasonably expect the vaso-motor nerves to exert on this process? Manifestly only an indirect one. By causing contraction or enlargement of the small blood vessels, the amount of blood admitted to a given part may be increased or diminished, and in this way alone does it seem possible for these nerves to alter nutrition.

But what are the views of M. Vulpian on this subject? In its discussion he refers first to the structural alterations which nerve fibres undergo when they are divided, as was first noted by Waller. This leads to an elaborate discussion in respect to the histology of nerve fibres, with special reference to his own researches and those of M. Ranvier, (*Archives de physiologie*), and which, taken altogether, is one of the most luminous we have yet met with. But it is, beside the plan of this notice to enter on these histological details, however interesting in themselves, or necessary to the purpose of M. Vulpian. He doubts the existence of a true axis cylinder in the pale fibres of Remak.

He also affirms the existence of the much disputed *perineurium*, first noticed by Robin, and in which he is at one with M. Ranvier, and the Swedish anatomists and histologists, Axel Key and Retzius.

A elaborate account of the structure of the white nerve fibres, in which he follows M. Ranvier, and lays especial stress on the "*annular strangulations*" which this histologist describes, as occurring at regular intervals along the course of the nerve fibres, and which he declares correspond to the points of apposition of the cells of which the fibres were originally composed.

M. Vulpian enters on a remarkably critical discussion as to the cause of the Wallerian degeneration in divided nerves, in which is a very graphic and intelligible account of the process, from its inception to its ultimate terminations, in destruction or regeneration. But we cannot give our readers a satisfactory *résumé* of this part of M. Vulpian's work. Nothing less than the full account would satisfy the inquiring reader. The

chief interest of the discussion for us at present is in relation to the nature and origin of the process or processes by which the nerve change is brought about.

If we divide the motor root of a spinal nerve, say midway between the points of its connection with the cord, and its junction with the sensory root, and then wait a few weeks and then examine the two ends of the divided nerve, we find the central end,—that is, the one attached to the cord, to be, in the main, in a healthy condition, while the peripheral end would be found ordinarily, to have undergone for a considerable distance, degeneration. The question arises why the part of the divided nerve fibres on one side of the cut degenerate, and on the other, they do not.

The same state of things is to be observed, in case of section of the posterior root outside of its ganglion. But if this root is divided between the ganglion and the spinal cord, the change takes place in the part which is attached to the cord, while the part which lies to the peripheral side of the section, or towards the ganglion, is preserved.

According to Waller, each nerve fibre has one of its ends connected with a "nutritive centre," which for the motor roots of the spinal nerves at least, seems to be seated in the gray matter of the spinal cord. If the results of experiment may be trusted, the fibres of the posterior or sensory root, have their nutritive centre in the ganglion on their course. For if the sensory root is divided outside of the ganglion, the portion attached to it does not degenerate, while the other does, and the same is true, if the sensory root is divided between the ganglion and cord, viz., the end attached to the ganglion does not degenerate, but the central one attached to the cord does.

M. Jaccoud, holds that the degeneration of the nerve fibre depends simply on "functional inertia." This view is also shared by many others. But M. Vulpian very properly—so it seems to us—rejects this view. For not to mention anything farther at present, how is it, that the fibres of the sensory root, degenerate in contrary directions when divided on opposite sides of the ganglion on the sensory root?

The view that it may depend on altered vascularity of the affected nerves, is also, and very properly rejected, and that one is adopted, which admits that the nervous system by means of the so-called "nervous influence," may exercise control in one way or another, over the nutrition of the tissues of the body. If this influence is exerted normally all other circumstances being equal, the structural integrity of the tissue is maintained, but if not so, then the intimate structure of the tissue suffers, in various degrees, and perhaps in various ways.

Waller held that not only the gray matter of the spinal cord, or some part of it, but also the spinal and many of the sympathetic ganglia exerted this so-called "trophic influence" on the nerve fibres. And this action is *exciting*, according to Waller.

But Bernard, and more recently M. Ranvier, hold that this influence is simply *moderating*, instead of exciting. The energy of nutrition according to these authors, belongs to the tissues themselves. The nervous system only guides and represses it. M. Vulpian adopts Waller's view with some hesitation. He adopts the view that the ganglia on the posterior roots of the spinal nerves, exercise in the normal state, an influence on the fibres of the sensory roots, on which the ganglia are found. But it appears to us, M. Vulpian has not sufficiently considered the fact, even if aware of it, that the fibres of the sensory root have no demonstrable anatomical connection with the cells of the ganglia in question. The nerve cells in these ganglia lie between the fibres in clusters, and give off fibres which appear to pass toward the periphery of the body, and not toward the cord. It would seem a difficult matter to reconcile the apparent action of these ganglia on the sensory fibres in these degeneration experiments, with their want of anatomical connection with the same.

This is a fruitful theme, because it affords the opportunity within a small compass, of examining and as we believe settling, certain very important physiological problems. But it is our intention soon to discuss this subject, at considerable length, in another place, and hence we dismiss it for the present.

The next subject considered, is that of the alteration which occurs to a muscle, apparently as a result of a lesion of its nerves.

This subject has acquired extraordinary importance, in the past few years, in consequence of its relation to progressive muscular atrophy, and kindred disorders. Pathologists are even now divided into at least *two* classes, in respect to the relations of the nervous system to lesions of nutrition in the muscles. One party holds, that disease of the anterior horns of gray matter in the spinal cord, may cause trophic change in related muscles, through the medium of connecting nerve-trunks.

An influence of some kind passes from the seat of disease, in the anterior gray horns, for example, along to the related nerve fibres to the muscular fibres in which they terminate, which leads to a change in the rate and character of their nutrition.

The other class, hold that if trophic alterations occur in the muscular fibres, they do so either without regard to the state of their nerves, or as a primary muscular disease, or that if it arises apparently consecutive to disease of the related nerves, it is because the morbid action is first set up in the nerve fibres, and following along these, is transmitted by simple extension, to the muscular fibres, to which the nerve-fibres are distributed. These pathologists deny that any "nervous influence" passes along a nerve fibre which can directly change the rate or character of nutrition in a part. Without going into this question at greater length, we may say that among those of the

second class, as well as the first, some are found who hold that the changed nutrition of a muscle which seems to depend on certain nerve lesions, is really due to changes in the quantity of blood circulating in the muscle. It is an affair of the vaso-motor nervous system. Such persons hold, that it is not necessary to suppose either that a nervous influence has passed along the nerve fibre, capable of disturbing its nutritive activities, or that there has been an extension of organic disease, smouldering along the course of a nerve fibre, so as to at last invade the anatomically related muscular fibres.

It is alone needful to admit more or less blood to the affected muscle, and in this way the marked changes can be produced.

M. Vulpian enters on a very exhaustive discussion of this case, and relates many experiments, made with the view of settling this question. From every point of view, he maintains that the nerves which proceed to the muscles,—to say nothing of those leading to other organs and tissues,—may become the channels of influences, disturbing to their nutrition, just as truly as they are of influences, which when excited, cause contraction of the muscular fibres. No one thinks of denying this latter, and as it seems to us, cannot successfully deny the former.

This is in perfect accordance with views which we have long held, and have sometime since, published.

In this connexion M. Vulpian discusses the trophic disorders, which take place in the hind legs of various animals, after section of their sciatic nerves, and also the alterations which sometimes occur in bones, as the result of lesion of their nerves. He rejects the hypothesis that it depends wholly on a vaso-motor, or circulatory disturbance, as inadequate to explain all the phenomena. He would admit trophic nervous action, or rather the absence of it, as one of the factors in the case, and as we think, with eminent propriety.

The next subject for consideration is the effects on the eye of intra-cranial division of the trigeminus. These in the main have been more or less familiar to students of physiology from the days of Magendie. M. Vulpian gives pretty thorough discussion of the results arrived at by others, and certain results from his own experiments, all of which tend to show that this nerve contains trophic fibres, while the organic changes in the tissues of the eye may be partly at least, ascribed to trophic action.

The structural disorders of the kidney, which appears to follow section or cauterization of its nerves, depends in the opinion of our author, partly on a failure of nervous trophic action and partly on the accidental violence which the vessels of the kidneys may have suffered during the operation to which they were subjected.

At this point M. Vulpian is led to speak once again of the effects of section of the vagus, on the lungs. Readers of our last number, will remember that this subject was discussed in the

earlier part of this review. There seems to be no adequate reason for noticing it again in this place, farther than to say that this time he quotes the paper of Genzmer, to which we referred at that time, as being one in which his view was disproven. But he re-affirms his former opinion, viz.: that of Traube, of Berlin. He believes the changes which occur in the lungs to be partly trophic. Under this head of trophic influence of the nerves, he would range certain atrophies which have been observed in the testicles, as by Obolensky and Schiff, and in the comb and wattles of the cock, such as have been experimentally produced by Legros, Schiff and others, and which changes follow lesion of the related nerves.

The alleged atrophy of the brain is referred to which was first observed by Brown-Sequard, to follow section of the cervical sympathetic. In 1872, this latter physiologist exhibited to the Biological Society at Paris, two brains from Guinea pigs, both of which had suffered section of the cervical sympathetic on one side. At the end of about eight months, the animals were killed, and the half of the brain corresponding was found atrophied, while, on the contrary, the bones of the head were enlarged. Admitting the fact, the next question of importance is, how was the atrophy produced? M. Vulpian admits the brain atrophy, but has not been able to produce experimentally hypertrophy of the skull bones, as in the cases of Dr. Brown-Sequard.

The atrophy of the brain, is ascribed to a contraction of the cerebral vessels on the affected side, by which the supply of nutritive materials is relatively diminished, and hence, the atrophy. Cases in which this result does not follow division of the cervical sympathetic, are to be ascribed, according to M. Vulpian, to some anomaly of nervous distribution.

Our author now passes to an elaborate review of cases experimental and accidental, which go to show that the nervous system really exercises an influence over the nutrition of the tissues of the body, as in the cases of ulceration following injury to the spinal cord in Guinea pigs, as was noted by Brown-Sequard, M. M. Pierret and Vulpian, or by M. Prevost on rats, M. Joffroy on dogs, etc., and in many instances in man, where rapid lesions of certain portions of the body have followed accidental injuries of the cord. Such cases have been reported by many different observers. Rapid alterations of the muscular and other tissues, have also been observed to follow closely on disease of the cord. Such cases are now very numerous in the annals of neurological science. M. Vulpian is not one of the number, who would explain the occurrence of the peripheral lesion, say of a muscle as due to a mere extension of a neuritis along the nerve trunks from the seat of disease in the cord, for example to the affected muscles, so that the latter become diseased by mere extension. In his opinion, the trophic change in the muscles or other parts, are due to the withdrawal of their stimulating trophic action.

The results of this study are carried over to the elucidation of the phenomena of infantile spinal paralysis, and related forms of spinal disease in the adult. But we cannot tarry in this place to canvass this subject as it deserves, and this is hardly necessary, since the same subject has been treated by us at length elsewhere in our Journal. Besides this, such disorders cannot be considered as vaso-motor troubles of the cord, only in slight degree, and hence do not fall within the scope either of this notice or the work of M. Vulpian. But we do not complain that our author transcends the strict limit of his work in introducing matter which is not legitimately included in it. Fully one-half of these two goodly volumes is composed of materials which do not concern the vaso-motor nervous system. M. Vulpian makes this work the occasion for delivering important information and discussions on almost every part of neurological science, except that which relates to the brain.

In glosso-labio-laryngeal paralysis, in progressive muscular atrophy, in affections of the nervous centres, etc., he holds that the muscular lesion is due to disordered trophic nervous action. He denies, however, the actual existence of trophic nerves, such as were contended for by Samuel. Trophic action is exerted through the medium of the motor nerves themselves, he thinks.

In respect to *unilateral facial atrophy*, after critically reviewing the works of Lande, Fremy, Guttman, Baerwinkel, Emminghaus and others, he denies that it depends on vaso-motor disorder, but cannot decide from the standpoint of present information, neither the nature nor the seat of the disorder, though it is admitted to belong to the sphere of the trigeminus, especially its upper two branches.

We have not the space nor time at present necessary for the discussion of this subject, but we deem it possible to arrive at a closer knowledge of the morbid process in this disease than has been presented to us by M. Vulpian.

Attention is next given to what have been called *reflex muscular atrophies*. These usually occur in connection with, and often, as apparent consequences of, neuralgias.

Nothnagel, of Jena, supposes that the painful sensations conveyed to the cord along the neuralgic nerve are reflected out along vaso-motor nerves to the vessels of the muscles, causing them to contract, and hence by diminishing the blood supply, causing the atrophy, while others have supposed, that by the same means a contraction of the vessels of the cord occurs, at or near the point in it, at which the affected sensory nerves are implanted, and hence the nutrition of the cord is diminished at the point in question, and hence there is a diminution in the energy of its trophic action on the affected muscles, and hence the consequent lesion. Again, Brown-Sequard has supposed that the irritative influence conveyed along the painful nerves to the cord, is reflected outward, so as to disturb the nutrition of the affected muscles, and thus to produce the lesion.

But M. Vulpian rejects the hypothesis which would explain the muscular disorders occasionally connected with neuralgias, by a reference to reflex action. And we are quite prepared to agree with him. He is disposed to attribute both the neuralgia and the muscular disorder to disease of the gray matter of the cord at the points of implantation of the related nerves.

We have long ago insisted on this point in respect to all neuralgias. But important as this point is, in various applications, we cannot do more than strongly commend it to the attention of our readers. It has been often referred to in the pages of the JOURNAL.

In the next, or twenty-fourth lecture, the important subject of the vaso-motor nerves, in the production of congestion and inflammation, is discussed at length. But it will not be possible for us in this number to review it in an adequate manner. We will postpone our remarks, therefore, until the next issue, in which it is our intention to carefully consider the whole subject of congestion and inflammation, as affected by the nervous system. We can promise our readers that the most interesting and practical part of this great work of Prof. Vulpian lies in the latter half of the second volume.

(To be Continued.)

V. AMERICAN NEUROLOGICAL ASSOCIATION.

TRANSACTIONS OF THE AMERICAN NEUROLOGICAL ASSOCIATION FOR 1875. Edited by F. P. Kinnicutt, M. D., and T. A. McBride, M. D. Vol. I. New York 1875, 257 pages.

Readers of the JOURNAL have been made familiar already with the organization and plan of operations of the American Neurological Association. A rather full account of the proceedings, at the first meeting, was published in the July number of our last volume.

The organization was somewhat suddenly effected, in conformity with a call, issued less than six months prior to the meeting, and the visible results are contained in this goodly volume of 254 pages, which we have no hesitation in saying, is fully equal to the published work of any other American Medical society, that has appeared during the past year, notwithstanding the fact, that it relates to the initial meeting, and the fact of the smallness of its list of members.

Besides an account of proceedings, it contains 21 papers, the